Moreover, the author on p. 183 says that London coal gas has an illuminating value of 16 to 17 candles, and a calorific value of about 668 B.T.U.'s, which is much more nearly true for the gas supplied by the Gas Light and Coke Co. The error is of importance, as an engineer working at the problem of the gas-engine and consulting records of efficiency made with London gas might be seriously misled.

In dealing with water gas, no mention is made of the more modern processes such as the "Dellwik," now so largely used for the production of blue gas for welding, as well as for diluting coal gas.

It is admitted in the preface that the article on practical photometry has not been brought up to date, and this is a pity, as more than seven pages are devoted to the Bunsen photometer and the manipulation of candles, now practically extinct in all but name as a standard of light, whilst a couple of pages on pentane standards would have been of real value.

In spite of a few blemishes, the whole work is so good that no engineering chemist can afford to be without it.

Die Photographie im Hochgebirg. By Emil Terschak. Second edition. Pp. xxiii+62. (Berlin: Gustav Schmidt, 1905.) Price 2.50 marks.

EVERYONE who is of a roving disposition, and takes his camera to Switzerland or the Tyrol, or any other region where mountain climbing is pursued, should, if he wishes to gain by the experience of others, read this book. It is written by a photographer to photographers, and is not only very interesting to read, but contains a great amount of very useful photographic information of a particular kind.

The successful photography of mountain scenery, of ice, snow, and clouds at high altitudes requires not only forethought, but much experience. As it is necessary to carry all the apparatus that is required, the equipment must be well attended to, and since also one does not necessarily wish to climb high altitudes to take again a particular view that has not turned out photographically successful, one must be sure of securing a good negative at every exposure.

The first edition of this book appeared in 1900, but the author has since gained much more useful knowledge, which he has embodied in the present edition. The book is clearly printed in Roman characters on good paper, and the illustrations are numerous and well reproduced.

The Royal Medical and Chirurgical Society of London. Centenary 1805–1905. Written at the request of the President and Council by Dr. Norman Moore and Stephen Paget. Pp. 337. (The Aberdeen University Press, Ltd., 1905.)

THOUGH not the oldest of the medical societies of London, the Royal Medical and Chirurgical Society holds a position second to none, and the present volume of chronicles will not only be welcomed by its Fellows as giving a history of their society, but forms a useful record of the art and science of medicine during the nineteenth century, with comments by the compilers. A noteworthy feature of the volume is the list which is given for each year of the principal papers read before the society, both published and unpublished, extracts being given from the more important ones. Thus, for the year 1833, we find Hilton's unpublished account of *Trichina spiralis* in human muscle, which ante-dated Paget's discovery of this parasite. Short bibliographies of all the presidents and a full index complete this interesting volume, which contains several illustrations of the various premises occupied by the society and a photogravure frontispiece of William Saunders, the first president. **R. T. HEWLETT.**

LETTERS TO THE EDITOR.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. *No notice is taken of anonymous communications.]*

Tidal Researches.

IN NATURE for January 11 (p. 248) appear some criticisms upon my paper entitled "Cotidal Lines for the World."

The critic says :—"The construction of these charts is, unfortunately, but vaguely indicated." In reply to this it may be said that the charts embody all data known to me at the time of their construction, and to such data references as copious as space seemed to permit are given. What is meant by cotidal lines is given in § 17. Notions relating to the local modifications or peculiarities of cotidal lines have been given in considerable detail by means of lemmas and examples. In the construction of these lines, large detailed charts showing soundings wherever known were employed, and these depths were carefully considered in each step of the process. The ranges of tide written along the shore-lines simply re-present data, and in no way depend upon any theory or hypothesis. The same is essentially true of the cotidal lines where observations or data are sufficient. Wherever harmonic constants are available, the length of the series analysed is of secondary importance in the construction of cotidal lines, the results from two months being about as satisfactory as those from twenty years.

If we are not permitted to extend cotidal lines outward from the shore, we might about as well draw them upon the land as upon the water, for in either case they would only serve to point out the shore values. The reviewer thinks well of Berghaus's chart, and so do I. However, it is difficult to believe that a philosophical critic could long rest content with cotidal lines extending but a short distance off shore, and forming no connected or consistent system. Of course, the attempt, on my part, at covering all seas does not imply that all charts are equally good. In some instances the data were very meagre, and attention was directed to this fact more than once in the paper.

It seems strange that any serious misunderstanding could exist in reference to the method employed in inferring the times when the water particles are at elongation in particular directions. Does anybody doubt the conclusions reached in § 56, part iv. A? If these conclusions are wrong, let us hear the correct ones. If §§ 60–65, part iv. A, are not clear as they stand, it seems as if § 24, part iv. B (to say nothing of a reply to former criticism, NATURE, April 23, 1903), ought to remove all obscurity.

Perhaps the following remarks may be of some service in this connection :—

Unless the free period of a body of water, or of some portion of this body, approximately agrees with the period of the tidal forces, the tide in the body proper must be small, and generally smaller than the theoretical equilibrium tide for the body in question. But in many parts of the oceans the tide is several times greater than that which could be raised by the forces, even if we could suppose sufficient depths and sufficiently complete boundaries for enabling equilibrium tides to occur. Hence regions the dimensions of which approach critical values must exist in the oceans and account for the principal tides. If the aërial vibrations accompanying a musical tone act upon a series of resonators suited to various pitches, the one or more constructed for the given tone will respond to it, while all others will be practically silent; that is, the dominant impressed motions belong to resonators having critical dimensions, and not to the resonators in general.

That stationary oscillations of unexpectedly large amplitude exist in the oceans there is abundant evidence. In fact, a glance at the charts under criticism will show regions of large ranges over each of which the time of tide varies but little. As a nodal line is approached the range diminishes, and the time of tide changes rapidly in a comparatively short distance. Moreover, the dimensions of the oceans are such that areas having nearly critical

lengths can be readily discovered; these respond well to the forces, and their tides must be the ruling semi-diurnal tides of the oceans. The charts prove the existence of large stationary oscillations. To doubt this fact would be scarcely more reasonable than to doubt the existence of the tide itself. The large ranges of tide imply critical lengths, and critical lengths imply that the phase is controlled by the resistance to the movement.

In a second approximation it may be possible to take into account the actual departures from critical lengths, and to make some numerical estimates of the resistance, and to fix more accurately the modes of oscillation having regard to the deflecting force of the earth's rotation. In my paper the latter effect has been considered only in reference to arms or bodies of water tidally dependent upon larger bodies.

As soon as my critics develop their tidal theories sufficiently far for making definite suggestions, I shall be pleased and bound to give such developments careful consideration. In the meantime, I believe that nothing is gained by criticism which does not constantly revert to such facts as have been brought out through observations upon the tides. These constitute the final test of all theories.   R. A. HARRIS.
Washington, D.C., January 26.

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It is Mr. Harris's theory with which we were, and continue to be, at variance. We were unable to gather the part played by this theory in the construction of his series of cotidal charts, and hence our statement that this construction was but "vaguely indicated"; but we are glad to be assured that the theory has only been employed in regions where observational data were entirely wanting, and has not been allowed to vitiate, as we feared, results obtained direct from observation.

In reference to the phase theorem which we selected for special comment, Mr. Harris now states that "the large ranges of tide imply critical lengths, and critical lengths imply that the phase is controlled by resistance." The latter part of this theorem we are not prepared to admit unless it be further contended that the critical conditions implied are mathematically *exact,* especially in consideration of the comparatively small frictional influences which can be brought to bear on the motions of the sea. Any departure from the ideal critical state, and we contend that such departures must inevitably occur in a complex system like that of the ocean, will render the determination of phase dependent on such departures as well as on frictional influences, and we differ from Mr. Harris in regarding the former rather than the latter as the more powerful controlling influence in regard to phase. Whence can the large resistances to motion, implied in Mr. Harris's theory, arise?   S. S. H.

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Atomic Disintegration.

ACCORDING to the investigations on radium, especially by Prof. Rutherford, there can be no longer any doubt that the formation of helium from radium is due to spontaneous disintegration of the radium atom, and it is the same with the other radio-active elements. Most competent investigators have not hesitated to apply the same point of view also to all the other elements.

The enormous amount of energy set free in the formation of helium—about 10^9 great calories for a gram-atom of helium—must render hopeless any attempts to reverse this process. Considering the conformity of the other gases of the helium type—neon, argon, krypton, and xenon—it seems probable that they owe their existence to a similar disintegration of atoms. It is not surprising, therefore, that all attempts have failed to obtain a chemical compound of those gases, and I do not think such attempts likely to succeed in future. That, as yet, those gases, excepting helium itself, have not been recognised as products of atomic disintegration may be due to their difficult test.

Now it seems to me there is nothing contrary to the view that *disintegration of atoms is an irreversible process, strictly analogous to dissipation of heat.*

Considered in this way, there exists a parallelism not only as regards the first law of thermodynamics—conserv-ation of energy—with the principle of conservation of matter, but also regarding the second law—dissipation of energy, on the one hand, and atomic disintegration on the other. And as it has been stated by Clausius that the world's entropy tends towards a maximum, we may say that likewise the quantity of free helium and the similar "Edelgase" tends towards a maximum.

This parallelism in material and energetical law appears to me well worthy of notice.   W. MEIGEN.
Freiburg i/Br.

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Phosphorescence of Pyro-soda Developer.

SOME time ago (January, 1904) you were good enough to publish a note on the "Phosphorescence of Photographic Plates," and the following additional particulars of this phenomenon may be of interest. The developer used is the ordinary pyro-metol-soda solution.

If a bromide plate is exposed in the camera, developed, washed for a few moments only, and then placed in aluminium sulphate solution in the dark, the picture becomes luminous and shows forth as a *negative,* the high lights being dark, whilst the shadows are bright, the darkest ones phosphorescing most strongly. If, however, the plate (after having been exposed and developed) is washed thoroughly for half an hour by means of a jet of water under pressure, no phosphorescence is observed on treating it with Al_23SO_4 solution, from which it appears that a trace of the developing solution is necessary to cause phosphorescence in the plate.

If a few spots of unused developing solution are placed in the bottom of a porcelain dish and Al_23SO_4 solution is added (in the dark), the mixture will phosphoresce. But if the developer has been used for developing exposed plates, then its power of phosphorescence is weakened, and if the same portion of solution is used repeatedly for developing, and tested periodically for phosphorescence between the developments, it will be found that its phosphorescing power is diminished after each development, and that it finally vanishes altogether. This explains the production of the phosphorescing negative. The most strongly lighted part of the film is that which will destroy the phosphorescing power of the developer it has absorbed, and the unlighted portion or shadow is that in which the absorbed developer will be least changed, and therefore most strongly phosphorescent.

The addition of various substances to the aluminium salt modifies its phosphorescing power, and some prevent it altogether, even when added in very small quantities. Among those substances which strongly counteract the phosphorescence may be mentioned the salts of thorium, uranium, copper, lead, bismuth, iron, tin, cobalt, nickel, chromium, zinc, cadmium, mercury, platinum, and silver in the order named, while the salts of potassium, sodium, ammonium, lithium, calcium, barium, strontium, magnesium, and manganese seem to have little influence one way or the other.

The only substance found which has the effect of much increasing the brilliancy of the phosphorescence is gold. A solution of $AuCl_3$ alone, in fact, gives a more brilliant phosphorescence than Al_23SO_4. The gold is reduced to the black metallic form, and while this reduction is proceeding light is emitted. Other reducing agents, however, do not appear to emit any light during the process of reduction of $AuCl_3$. The influence of the other metals on the phosphorescing power of the gold solution seems to be practically the same as for aluminium.

Other aluminium salts, such as the nitrate, phosphate (dissolved in HCl), chloride, &c., phosphoresce with pyro-metol developer, but none so brilliantly as the sulphate.
                                             T. A. VAUGHTON.
Ley Hill House, Sutton Coldfield, February 13.

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Emission of Light by Kanal-strahlen Normal to their Direction.

IN a former publication (*Physik. Zeitschrift,* vi., 892, 1905) I have proved that the stream of positive ions which form the Kanal-strahlen show the Doppler effect. In these rays we have, therefore, a positive charge, and at the same time velocity, and also, as a result of the vibrations of the negative electrons, emission of light. Therefore it is